

9. Sur les chaleurs spécifiques des gaz sous des pressions plus grandes que la pression de l'atmosphère. Ce travail a pour but de décider si la chaleur spécifique d'une même masse de gaz reste constante sous les différentes densités que l'on peut donner à ce gaz, et de trouver la loi de la variation.

10. Sur les chaleurs dégagées ou absorbées par l'air pendant sa compression ou son expansion. Ce travail est celui qui m'a le plus longtemps arrêté et qui m'a empêché de publier l'ensemble de mes recherches. C'est à cette occasion que j'ai étudié les phénomènes calorifiques qui se produisent pendant le passage des gaz comprimés à travers de petites ouvertures ou par des tubes capillaires, suivant que le gaz est chaud ou qu'il est froid. J'ai étudié le même sujet sur le gaz qui produit un travail moteur—

1. Complètement intérieur au calorimètre.

2. Extérieur au calorimètre et produisant un effet mécanique qui puisse être assigné.

Je suis encore arrêté dans les conclusions à déduire de ce dernier travail, par suite de plusieurs anomalies, dont je ne trouve l'explication par aucune des théories proposées.

11. Sur les chaleurs spécifiques des vapeurs sous diverses pressions. J'ai soumis à ces recherches un très grand nombre de vapeurs, mais en insistant plus spécialement sur celles qui peuvent être obtenues à l'état de pureté parfaite, et hélas ! elles ne sont pas très nombreuses. La chaleur spécifique que j'ai trouvée à la vapeur d'eau sous la pression de l'atmosphère est 0.475 par rapport à l'eau liquide.

L'ensemble de ces recherches permettra de calculer numériquement les effets mécaniques que l'on peut retirer théoriquement de 1 kil. de charbon, suivant que l'on applique la chaleur à la production de la vapeur d'eau ou d'une autre vapeur, sous diverses pressions, à saturation ou sous détente, ou qu'on l'applique à un gaz quelconque à détente ou sous pression constante. En un mot, j'espère que les résultats obtenus dans ces recherches permettront de résoudre enfin, avec quelque certitude, les questions qui divisent depuis si longtemps les ingénieurs, en même temps qu'elle fourniront un grand nombre d'épreuves pour décider les diverses théories qui ont été proposées pour la chaleur.

Je vous prie, mon cher Colonel, de donner lecture de cette lettre à la Société Royale, et d'en demander l'insertion dans ses publications, si elle n'y trouve pas d'empêchement.

J'ai l'honneur d'être,

Votre dévoué serviteur,

V. REGNAULT.

2. Extract from a letter from M. Kämtz addressed to Colonel Sabine (Translation):—Received April 10, 1853.

Dorpat, 15th March (N. Style), 1853.

I have just completed the memoir on terrestrial magnetism which I mentioned to you in former letters. It would follow from the materials which I have employed, that the horizontal force requires constants which differ from those for the vertical force, and the

differences, although for the most part not considerable, are sometimes too large, as it appears to me, to be overlooked: it is for observers to decide in this matter. To avoid the introduction of insecure numbers I have not computed the potential, but have contented myself with deriving the values of X , Y and Z (declination, inclination, and force horizontal and total) being the quantities principally used for comparison. A second calculation of the constants for X and Y would have given rather more exact values, but I think that with the existing data the result would scarcely repay the labour of so extensive a calculation, for the tables would have to be entirely recomputed.

After obtaining the data from the theory, I wished to compare them with observation. I could take for the inclination and force the values which had served as the bases of the calculation; there are indeed several observations of inclination without our knowing the force, and a few of the force without inclination; but on the other hand, there are more extensive districts where I had no declinations, although the other two elements were well determined. Under these circumstances I was obliged to have recourse to older observations, which indeed I had occasion to use also for secular change and reduction for epoch to 1830. Thus the character of the work was gradually modified. For the Atlantic Ocean I availed myself of your excellent Memoir, Contributions No. IX.; I only added the observations of Lütke, d'Urville, and Rumker; of older observations, I took those of Abercrombie and Ekeberg, only using them however in parts where they had observed the inclination, and principally with the view of finding its secular change.

In the Pacific there was more deficiency; with the exception of what has been furnished by Lütke, d'Urville, the ships of the Prussian Merchant Service, and in the neighbourhood of the magnetic equator by Duperrey, modern voyages scarcely gave me anything; for Becquerel, in his detailed table*, omits all determinations made at sea. I therefore took all I could get since the voyages of Byron, Carteret and Wallis, chiefly from Hansteen's great work, and reduced to 1830, taking from each observer the mean of all his determinations within a space of 5° lat. and 10° long. I did not allow myself to exclude any observer, for as all have errors from the ship's iron, it might easily happen that a man but little known may have given values which are much nearer to the truth than those given by a celebrated voyager. A selection is no less objectionable, as it might easily lead to retaining observations which accord with a possibly not altogether correct theory, and omitting others, otherwise good, which might depart from it. Moreover, such a full comparison shows better what may be expected from the observations: incomplete as are the data they afford, yet, when I arrange the remaining errors in latitude and longitude, I rarely find groups in which the errors show a certain degree of systematic character over extensive districts, so as to alter the declination or inclination a degree.

* *Electricité*, tome vii.

The inclination appears to me to be very incorrect in the meridian of Alexandria. In India the few results of Elliot's given in the Phil. Mag. from the Proceedings of the Royal Society, accord extremely well with my calculations. I subjoin a few points of comparison:—

North Pole in $70^{\circ} 7' \text{ N.}$, $263^{\circ} 37' \text{ E.}$ Ross's observations give $70^{\circ} 5' \text{ N.}$, $263^{\circ} 14' \text{ E.}$ For the South Pole, $74^{\circ} 6' \text{ S.}$, $152^{\circ} 47' \text{ E.}$ seems to agree less closely with Ross's map, although the inclinations observed in the neighbourhood agree well with the calculated ones.

Maximum of total force in the northern hemisphere	1871.8 in $54^{\circ} 21' \text{ N.}$	$265^{\circ} 53' \text{ E.}$
(Error compared with Lefroy's observations	+6.4 in $54^{\circ} 1' \text{ N.}$	$260^{\circ} 1' \text{ E.}$
	+0.8 in $55^{\circ} 6' \text{ N.}$	$267^{\circ} 4' \text{ E.}$
	+4.6 in $50^{\circ} 2' \text{ N.}$	$264^{\circ} 0' \text{ E.}$

For the force in the southern hemisphere, 2027.6 in $64^{\circ} 36' \text{ S.}$, $144^{\circ} 34' \text{ E.}$ is perhaps less correct, but there are in the S. hemisphere some striking anomalies in the observations.

The probable error of the force is 19.1 ; if however some groups which are at any rate not very correct are omitted, it becomes a little less than 15 . For the inclination, 31.1 ; for the declination, 41.3 .

I have gained my principal object in the work by becoming myself better informed, but I have thought it might perhaps not be wholly useless to others if only to stimulate travellers to make observations for comparison, and I have therefore been at the pains of writing out the table of comparisons (a task more irksome than that of calculation). But to publish is very difficult; on such a subject, and under present political circumstances, no bookseller in Germany would undertake it. The Admiralty at St. Petersburg would perhaps print it in their Memoirs, but in the Russian language, in which it would be as good as buried; I turn to you, asking whether it might not be possible to get it published in England, not as a separate work, for perhaps no bookseller would accept it, but in a collection of similar papers. To me it would be most agreeable if it could appear either in the Reports of the British Association or in the Philosophical Transactions; and by preference in the latter, because I have derived from thence the best part of my materials, besides which the first results have already appeared in the Proceedings of the Royal Society. You will be best able to judge for me; I do not know what is the practice of either of those bodies in regard to writings not by their own members. My memoir is, I fear, long, but yet it consists chiefly of the table of comparisons. Of maps, I give declination, inclination and total force, two of each (Mercator and Polar). Unluckily I have no translator here who understands the subject, so I could only make the attempt myself, and as the memoir consists chiefly of figures from whence simple deductions are derived, there cannot be great errors; in the rest pray make the needful alterations where there are glaring Germanisms, if you think it possible the whole may be printed. If not, pray send me back the MSS. at some future time, not for publication, but for my own use. I finished yesterday, and send this week to the British Embassy; only I repeat my prayer,

that you will receive the work kindly, and tell me of its fate as soon as you can.

How far advanced is the Magnetic Survey of India? Could not you get travellers to observe in Egypt, Nubia, Mesopotamia and Persia? In the Pacific we want observations, especially on Erman's route: the number of facts known is too small for good inferences: we want tests for Duperrey's and Freycinet's Dips, which at neighbouring points differ 3° or 4° . Of Elliot's I know only the results taken from the Proceedings of the Royal Society; Kupffer has, I see, a copy of the work itself from the Phil. Trans. Has not Rae made observations on his journeys, and is anything printed? the same question as to all the expeditions in search of Franklin?

(Signed)

L. M. KÄMTZ.

April 28, 1853

The EARL OF ROSSE, President, in the Chair.

A paper was read, entitled "On the Application of the Law of the Conservation of Energy to the Determination of the Magnetic Meridian on board Ship, when out of reach or out of sight of Land." By W. J. Macquorn Rankine. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S. &c. Received April 5, 1853.

The author states that, assuming that when a ship is swung completely round, so that her head bears exactly as it did at first, the magnetism of the ship, and that of the compass-needle return to their original condition, the following theorem is necessarily true:—

The mechanical power developed by the mutual action of the ship and of the compass-needle during a complete revolution of the ship, is equal to zero.

If ζ' be the apparent magnetic azimuth of the ship's head, east of north; α' the corresponding apparent magnetic azimuth of a distant fixed terrestrial object (or where no such object is visible, of a star, corrected by calculation for its apparent diurnal motion); α the true magnetic azimuth of the same object, so that $\alpha - \alpha'$ is the westerly deviation of the compass-needle; then the above theorem is expressed symbolically thus:—

$$0 = \int_0^{2\pi} \sin(\alpha - \alpha') \cdot d\zeta' = \sin \alpha \int_0^{2\pi} \cos \alpha' \cdot d\zeta' - \cos \alpha \int_0^{2\pi} \sin \alpha' \cdot d\zeta';$$

from which it follows that

$$\tan \alpha = \frac{\int_0^{2\pi} \sin \alpha' \cdot d\zeta'}{\int_0^{2\pi} \cos \alpha' \cdot d\zeta'}.$$

The author remarks, that for the integrals in this formula are to be substituted, in practice, the algebraical sums of the sines and